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1593. Solanum Campechianum calycibus echinatis *H. Elt.* 360.

1594. Soldanella *Hort. Cliff.* Alpina rotundifolia *C. B. P.* 295.

1595. Sonchus floribus solitariis, foliorum laciniis extrorsum flexis *Hort. Cliff.* Sonchus Tingitanus papaveris folio *T.* 475.

1596. Teucrium foliis lanceolatis integerrimis petiolatis, spicis laxis subrotundis *Hort. Cliff.* Polium lavendulæ folio *C. B. P.* 220.

1597. Thevetia *Hort. Cliff.* Ahovas major *Pis.*

1598. Tordylium umbellulis remotis, foliis pinnatis pinnis subrotundis laciniatis *Hort. Cliff.*

1599. Veronica spicata lanuginosa & incana floribus cæruleis *Amman. Ruth.*

1600. Zigophyllum foliis subsessilibus *Hort. Cliff.* Fabago Africana arborescens flore sulphureo, fructu rotundo *Com. Rar.* 10.

LXX. *An Account of some Experiments upon a Machine for measuring the Way of a Ship at Sea. By Mr.* J. Smeaton, *F. R. S.*

Read April 4, 1754.

IN the *Philosophical Transactions*, N° 391. for November 1725. Mr. Henry de Saumarez gives an account of a machine for measuring a ship's way more exactly than by the log. This machine consists of a first mover, in the form of the letter Y. Upon the two arms of the Y are fastened two vanes, inclined in such a manner, that when the

the Y is hauled through the water by a rope, faſtened to the ſtem or tail thereof, it may turn round, and, of conſequence, endeavour to turn the rope round. The other end of the rope, being faſtened to the end of a ſpindle capable of moving freely round, will be made to do ſo by the rotations of the Y, communicated to the rope. A motion being thus communicated to a ſpindle within the ſhip, this ſpindle may be made to drive a ſett of wheel-work, which will regiſter the turns of the Y; and the value of a certain number of theſe turns being once found, by proper experiments, they are eaſily reducible into leagues and degrees, *&c.* The only difficulty then is, whether this Y will make the ſame number of rotations in going the ſame ſpace, when it is carried through the water faſt, as when it is carried ſlow. Upon this head Mr. de Saumarez, as well in the paper above-cited, as in a ſubſequent one publiſhed in *Philoſ. Tranſ.* N° 408. for March 1729. has given an account of ſeveral trials, which he has made of it, from which it appears, that this machine, in part, anſwers the end propoſed; and is, in part, defective: The errors of which he ſuppoſes to proceed from the ſinking down of the Y into the water, upon a ſlow motion; the axis of its rotation being then more oblique to the horizon than in a quick one.

In a machine, conſtructed like this, it is evident, that the end of the ſpindle, to which the rope is faſtened, muſt be of ſufficient ſtrength and thickneſs, not only to bear the force or ſtreſs, that the hauling of the Y through the water will lay upon it, in the greateſt motion of a ſhip; but alſo to bear the accidental jerks, that the waves will ſuperadd thereto.

The

The thicknefs of the fpindle then being determined by thefe conditions; it is alfo manifeft, that, to prevent the fpindle from being pulled out of its place by the draft of the rope, there muft be a fhoulder formed upon it, which muft be greater than the part of the fpindle before defcribed, for the fpindle to bear againft. The fize, that Mr. Saumerez propofes to give to his Y, is 27 inches the whole length; 15 inches for the length of the arms (which are to be opened to a right angle); 8 inches for the length of each vane; 4 inches and an half broad, and the ftems and fhank to be two-thirds of an inch thick. According to thefe dimenfions, the refiftance, that this part of the machine will meet with, in paffing thro' the water, will, in the fwift motions of the fhip, be very confiderable: confequently, the neceffary bulk of the pivot-end of the fpindle, and its fhoulder, will occafion a confiderable friction in the turning thereof, and retardation to the rotation of the machine.

To cure thefe defects, as much as poffible, inftead of the Y before defcribed, I made trial of a fingle plate of brafs, of about 10 inches long, 2 and an half broad, one-thirtieth of an inch thick, and cut into an oval fhape. This plate being fet a little atwift, and faftened by one end to a fmall cord, in the manner of the Y, is likewife capable of making a rotation, in being drawn through the water; but with this difference, that as this is but a fmall thin plate drawn edgeways through the water, its refiftance, in paffing through it, is much lefs; of confequence, a much fmaller line is fufficient to hold it, which again confiderably diminifhes the refiftance; and this, of courfe, proves a double diminution of friction in the

fpindle:

ſpindle: Firſt, as the preſſure upon it is leſs; and, ſecondly, as it allows the ſpindle and ſhoulder to be of a leſs diameter. To break the jerks of the waves; next to the end of the ſpindle I fixed a ſpiral ſpring of wire, to which the cord was faſtened; which, by this means, was capable of playing backwards and forwards, and giving way to the irregularities of the ſea: and, leſt the plate ſhould lay faſt hold of any thing, or any extraordinary jerk ſhould damage the ſpindle or ſpring, a knob, or button, was faſtened upon the cord, at a ſmall diſtance from the ſpring, which ſtopped upon a hole in a piece of wood, and prevented the ſpring from being pulled out to above a certain length; ſo that all addition of force, beyond this, could only tend to break the cord, and carry away the plate. The ſpindle, being thus guarded from accidents, will allow of a ſtill further diminution of its ſize; ſo that, at laſt, I ventured to make the ſpindle-pivot no more than one-twentieth of an inch diameter, and that of the ſhoulder one-eighth; being of tempered ſteel, and ſufficiently ſmooth. The hole, in which the pivot, and againſt which the ſhoulder worked, was of agate likewiſe, well poliſhed.

Being thus provided, in May 1751. I procured a boat, upon the ſerpentine river in Hyde-park, to try how far the turns of the machine would be conſiſtent with themſelves, when the ſame ſpace was meaſured over with the ſame, and with different velocities. The courſe was determined at each end, by obſerving the coincidence of two trees, in a line nearly at right angles to the river. We, however, rowed beyond the

 mark,

mark, that the machine might be in full play when the courſe was begun: The ſpindle was ſtopped at the beginning and end; the numbers read off, and were as follows:

The ſpace between the marks was, by eſtimation, about half a mile.

				Revol.
1ſt	rowing up the river, in	11 min.	the plate made	615
2d	down	14	. . . .	645
3d	up	18 and an half	.	612
4th	down	9 and an half	.	603
5th	up	18	. . .	620
6th	down	10	. . .	600

It is obſervable, that the greateſt difference, among the above obſervations, is between the 2d and 6th, being 645 and 600; the difference being about one fourth part of the whole; the times being 14 minutes and 10, both in going down the river: Whereas thoſe obſervations, which differ moſt in point of time *viz.* the 3d and 4th, being performed in 18 minutes and an half, and 9 minutes and an half, reſpectively; have their revolutions more nearly alike, being 612 and 603; which differ only by one ſixty-eighth of the whole. From theſe obſervations I was led to think, that the different velocities, wherewith a veſſel moves forwards, would make no material difference in the number of rotations of the plate; or, at leaſt, that thoſe differences would be leſs than the irregularities ariſing from other cauſes, even in trials nearly ſimilar.

The

The next trial of this machine was on board a ſmall ſailing veſſel, in company with Dr. Knight, and Mr. William Hutchinſon, an experienced ſeaman, and maſter of a conſiderable merchant-ſhip. Our expedition was upon the river Thames, and ſome leagues below the Nore. The intention of the trial here was, to find, in general, how far it agreed with the log, and how it would behave in the ſwell of the ſea; a compariſon with the meaſure of a real diſtance being here impracticable, on account of the tides and currents.

The method of trial was this: We ſuffered the whole log-line to run out, being 357 feet between the firſt knot and the end. The perſon, who hove the log, gave notice, at the extremes of this meaſure, that the perſon, who attended the dial of the machine, might ſtop the ſpindle at the beginning and end; while a third obſerved, by a ſeconds-watch, the time taken up in running theſe 357 feet. By theſe means, we were enabled to aſcertain the comparative velocity, wherewith we moved, and the number of turns of the plate at each trial, correſponding to 357 feet by the log; which, if the machine and log were both accurate, ought to have been always the ſame. The particulars of theſe experiments are contained in the following table.

Turns of the Plate.		Seconds of time during the running out of 357 feet of log-line
83	In the river at anchor by the tide	124
82	The ſame repeated	134
81	Sailing in the river	98
79	In the river at anchor by the tide	135
76	Sailing in the river	115
74	At ſea upon a wind	64
74	The ſame repeated	69
71	Sailing in the river	71
70	The ſame	66
70	Before the wind at ſea	77
70	The ſame	56
70	The ſame	52
66	Before the wind in the river	55
64	The ſame	53
64	The ſame	60
64	The ſame	43
63	At ſea upon a wind	53
62	The ſame	52
62	Sailing in the river	45

It appears from theſe trials, made in different poſitions of the veſſel with regard to the wind, both in the river and at ſea, as well by the tides at anchor, as in ſailing, that the turns of the plate, correſponding with the ſpace of 357 feet by the log, were from 62 to 83; and the times, in which this ſpace was run, were from 45 to 135 ſeconds; the greater number of revolutions anſwering to the greater number of ſeconds, or ſlower movement of the veſſel. Upon finding this conſiderable diſagreement between the log and plate, when ſwift and ſlow motions are compared, I did not ſuppoſe, that they proceeded from a retard-

retardation of the plate in ſwift motions, but from the hauling home of the log in ſlow ones. As for inſtance; the log, to do its office accurately, ought to remain at reſt in the water, whatever be the motion of the veſſel. But even the keeping the line ſtrait, and much more the ſuffering the log to haul the line off the reel (as practiſed by many), will make the log, in ſome meaſure, follow the veſſel, and will be greater, in proportion as the time of continuance of this action is greater; and therefore the log will follow the ſhip twice as far in going one knot, when the ſhip is twice as long in running it. The conſequence of this is, that a veſſel always runs over a greater ſpace than is ſhewn by the log-line; but that this error is greater, in proportion as the veſſel moves ſlower. It is this reaſon, I ſuppoſe, that has induced the practical ſeamen to continue the diſtance between their knots ſhorter than they are directed by the theory.

Afterwards, in the ſame ſummer, I made ſuch another expedition, in a ſailing veſſel, along with captain Campbell of the Mary yacht, and Dr. Knight. Having prepared two of theſe machines as near alike as poſſible, I determined to try, how far they were capable of agreement, when expoſed to the ſame inconveniencies, and uſed together. During the trial of theſe machines, one made 86,716 revolutions, and the other made 88,184. During this ſpace, they were compared at ten ſeveral intervals. The revolutions between each interval differed from the proportion of theſe numbers, in the firſt compariſon, one-nineteenth of the whole interval. The errors of each interval, in the other compariſons, were, in order, two-ſeventeenths, one-nineteenth, one-twentieth, one-

 fifty-

fifty-fourth, one-fourteenth, one-eightieth, one-ſixty-ſeventh, one-fourteenth, one-ſixteenth; the greateſt errors being where the ſpaces were the ſhorteſt. In other reſpects, the plates ſeemed to perform their duty, in the water, well enough, tho' the ſea was as rough, in this voyage, as our ſmall veſſel would well bear.

Laſtly, Being, for ſome time, on board the Fortune ſloop of war, commanded by Alexander Campbell, Eſq; in company with Dr. Knight, for the purpoſe of making trial of his new-invented ſea-compaſſes, I had frequent opportunities of making uſe of theſe machines, by comparing them with one another, with the log, and with real diſtances; and having, by repeated trials, pretty well aſcertained the number of turns of the plate, that was equal to a given ſpace, by the help of the log, in the manner before deſcribed, when the ſhip was upon a middle velocity; I found the ſpaces, ſo meaſured, nearly conſiſtent with themſelves, and with the truth: But all this while the winds and weather were very moderate. It afterwards happened, that we run 18 leagues in a briſk gale of wind, which, tho' not fair for us (being before the beam), yet drove us ſometimes at the rate of 8 knots an hour, as appeared by heaving the log. During this run I obſerved, that the reſiſtance of the water, to the line and plate, was very conſiderable, and increaſed the friction of the ſpindle ſo much, as to prevent it from beginning to turn, till the plate had twiſted the line to ſuch a degree, that when it did ſet a going, it would frequently run 150 or 200 turns at once. I alſo obſerved, that the wind coming acroſs the courſe of the ſhip, blew the cord a good deal out of the direction of the ſpindle, and cauſed

caused the line to rub against the safeguard-hole, for the button to stop against, as above described; which undoubtedly occasioned considerable friction in that place. But the most untoward circumstance, that I observed, was, that being in a rough, but short chopping sea, and sailing obliquely across the waves, the plate would frequently be drawn from one wave to another through the air, without touching the water; and, as it appeared, would jump from one wave to another, the unevenness of the surface, joined to the quickness of the motion, not permitting the plate to follow the depression of the water. This evil I endeavoured to remedy, by placing upon the line, at a small distance before the plate, some hollow bullets, such as are made for nets, in order to keep the plate so low down in the water, as to be below the bottom of the waves. This, in part, I found they did; but they, at the same time, added so much resistance, in their passing through the water, that the inconvenience was as great one way, as the other.

Upon making up the account of this run, I found the number of rotations were less, by one full third, than they ought to have been, compared with former observations; which afforded me a convincing proof, that this instrument was considerably retarded in quick motions

The length of the line made use of was about 20 fathoms, which I found necessary, that the water, disturbed by the body of the ship, might be tolerably settled before the plate was drawn through it; but this length of line was also an inconvenience, as it met with greater resistance in the water.

Upon the whole, it ſeems to me, that an inſtrument, made as above deſcribed, is capable of meaſuring the way of a ſhip at ſea, when its velocity does not exceed 5 ſea miles an hour, to a degree of exactneſs exceeding the log. It therefore may be uſeful in the menſuration of the velocities of tides, currents, *&c.* and alſo in meaſuring diſtances at ſea in taking ſurveys of coaſts, harbours, *&c.* Thus far it ſeems capable of performing, upon the ſuppoſition, that it cannot be brought to a greater degree of perfection. But this I am very far from ſuppoſing: On the contrary, I do not deſpair, that it may be brought to anſwer the end of meaſuring the way of a ſhip at ſea univerſally; and, for that reaſon, it may not be amiſs to put down a few hints, concerning the cauſe and cure of the errors above-mentioned, for the ſake of thoſe, who may hereafter be inclined to proſecute theſe enquiries*.

It appears then from the preceding obſervations, that the rotation of the plate is conſiderably retarded in the quickeſt motions of the ſhip; and ſenſibly ſo, in all velocities exceeding 5 miles an hour. This may proceed, firſt, from the friction of the machine increaſing in a greater proportion than the power to turn it round. Secondly, From the water's being put in motion by the ſhip, ſo as to follow it in the ſame

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* Upon communicating theſe experiments and obſervations to my ingenious friend Mr. William Ruſſel, he gave me an account of a machine, that he had made trial of in a voyage, ſome years ſince, from the Levant, ſo nearly agreeing with the above-deſcribed, that one would have imagined we had been of each other's council in deſigning them.

di-

direction, and that to a considerable distance astern. And, Thirdly, from the plate's jumping from wave to wave, when their concavity is great, and distance little.

The first may, in some measure, be helped, by applying a loaded fly, of a proper size, to the spindle of the machine, which will prevent its sticking fast for a time, and then whirling round with great rapidity, as it is apt to do when the resistance is great; by which means, the motion will be rendered more equal and uniform, as was justly observed to me by my friend Mr. Ellicott of this Society.

Also, if the body of the machine were hung, equally poized, upon cross-centres, like those used for sea-compasses, or in the manner of a swivel-gun, as captain Alexander Campbell well proposed; the spindle of the machine would readily place itself in the same direction with the line that draws it, and thereby avoid unnecessary frictions from the oblique direction of the cord.

The second may be helped by placing the machine upon the end of a pole, fastened near the forecastle, over the side of the ship. By this means, a shorter line will be necessary, and the plate prevented from working in the more disturbed water at the stern.

Lastly, Its quitting the water, perhaps, might be helped by joining a shank of brass, of six inches long, and three-quarters of an inch diameter, to the fore-part of the plate, to which the cord must be fastened, the ends of the shank being formed into a figure most convenient for passing thro' the water with ease. The weight of this will cause the fore-part of the plate to

ſink faſter than the other, and endeavour to give it a direction down into the water *.

I had intended to have made trial of the effect of theſe alterations, but have been prevented, partly by want of opportunity, and partly from the indifference, with which I found ſuch a contrivance as this, even if brought to perfection, was likely to be received by ſeamen; who, in general, do not ſeem to be over-fond of making trial of new inſtruments, eſpecially if propoſed by landmen, as, in deriſion, they are pleaſed to call us.

Indeed it may be objected, that, could we meaſure the way of a ſhip thro' the water ever ſo exactly, unleſs ſome method were found out, of aſcertaining the currents, *&c.*; a ſhip's courſe, with reſpect to the globe, could not hereby be determined. But then it may be replied, with equal juſtice, that it is for want of a means of meaſuring the way of a ſhip thro' the water (and this compared with other check obſervations), that the drift and velocities of the principal currents have not already been determined.

Mr. de Saumarez, in his ſecond paper, of March 1729. makes mention of another machine for this purpoſe, which he himſelf acknowleges to be inferior to his former, eſpecially in rough weather at ſea. But as ſeveral others have fallen upon, and propoſed, a machine ſimilar to this; it may not be amiſs to add the following remarks upon it. The firſt mover, in this, is compoſed of four arms, fixed to the bottom of a perpen-

* Mr. Ruſſel's plate was joined to a ſhank, who never found it to jump out of the water, at any time, when he made uſe of it.

perpendicular ſpindle; each arm is furniſhed with a vane, which opens one way, and ſhuts the other, as ſome have attempted the making of horizontal wind-mills. This, by being carried thro' the water progreſſively, will turn round, and the faſter, as the ſhip moves faſter: But to judge, whether it will do it proportionably in all velocities of the ſhip, let us conſider,

1. That a good ſailing ſhip will frequently ſail at the rate of 10 ſea miles (60 to a degree) an hour, which is at the rate of 17 feet *per* ſecond.

2. Suppoſing the ſide of the fly, where the vanes are cloſed, to be retained by the water at reſt; the oppoſite ſide of the fly, where the vane is open, muſt meet the water with a velocity double to that of the ſhip, or at the rate of 34 feet in a ſecond; as would be the caſe with the upper part of a coach-wheel, whoſe velocity thro' the air is double to that, wherewith the coach moves forward.

3. That a plane ſurface of 3 inches ſquare, moving thro' the water with a velocity of 34 feet *per* ſecond, will meet with a reſiſtance, at leaſt, equal to 70 pounds avoirdupoize.

4. That the reſiſtance, which the open vanes will meet with in the water, will, in ſwift motions, be very conſiderable, and, of conſequence, the fly will move much ſlower than it ought to do, if theſe reſiſtances were leſs.

5. That from hence there is much reaſon to doubt, whether the reſiſtance of the medium, and friction of the machine, taken together, will always produce ſuch diminution, in the number of turns, as that the number of revolutions, actually ſhewn by the indexes,

may be the ſame, when the ſame ſpace is gone over with a great velocity, as with a ſmall one.

LXXI. *Obſervationes Eclipſium Satellitum Jovis habitæ Uliſſipone in Regali Collegio Beatiſſimæ Virginis à neceſſitatibus, dictæ à Joanne Chevalier, Præſbytero Seculari Congregationis Oratorii, Anno* 1753.

Read Feb. 14, 1754.

DIE 30 Aprilis, cœlo clariſſimo, obſervavi, teleſcopio Gregoriano 6 pedum longitudinis, emerſionem primi ſatellitis è umbra Jovis, hora poſtmeridiana temporis veri 9^h $57' \, 48''$.

Die 24 Maii, iterum cœlo puriſſimo, obſervavi, eodem teleſcopio Gregoriano, emerſionem tertii ſatellitis ab umbra Jovis, hora poſtmeridiana temporis veri 8^h $19' \, 6''$.

LXXII. *Obſervatio Solis defectus Uliſſipone habita, in Æde Beatiſſimæ Virginis à neceſſitatibus, nuncupatæ à Joanne Chevalier, Præſbytero Congregationis Oratorii, die* 26*ª Octobris* 1753.

Read Feb. 14, 1754.

HUNC ſolis defectum commodè, ac exactè obſervare potui, cœlo clariſſimo ac puriſſimo. Initium ac finem eclipſis teleſcopio 15 palmorum examinavi, digitos vero obſcu-

rationis